



11 August 1987

To Harriet Zuckerman and Josh Lederberg

Dear friends,

It was great to be able to spend an evening with you (Harriet) and it was good to be able to see you (Josh) at the Harvard Commencement, but all too briefly.

I am writing to you about your "commentary" on "postmature scientific discovery." I don't know when I have read an article relating to the history of science that has so greatly stimulated me as this one has done. ~~Not only is~~ The story itself absolutely enthralling, with many overtones about the nature of scientific behavior and, particularly, conduct, that may lead to real daring innovations. Even more, it strikes a deep resonating chord in my own thinking in relation to work that I am hoping to develop in the next phase of my own research.

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Let me ~~say you~~, at once, that ^{I find} the notion of something which is the opposite of "premature" ~~like~~ extremely important. You have, in fact, given me the key for understanding a very important topic which I am currently writing up.

Let me tell you about it briefly so that you will understand why your contribution has been so important for me. In my investigations of the nature of discoveries, their applications, their timings, and their reception, I have been putting together some materials on penicillin. In my book I date this discovery in the late 30s and early 40s, with the work of Howard Florey and his team, notably Chain. In considering this example, I make a distinction between Fleming's discovery of the bacteriolytic action of the mold and the later isolation of the chemotherapeutic agent and the production of a naturally produced antibiotic. In particular, I am interested in the question of dates. Here we have a wonderful example of your own category, since all (or at least almost all) of the technical support used by Florey was available at the earlier date to Fleming. In an older publication of mine, published in the 1940s, I explored some of the reasons why Fleming could hardly have been expected to



PROF. I. BERNARD COHEN · 22 Gray Gardens East · Cambridge, Massachusetts 02138

B. I. Cohen

follow up on his finding concerning the mold. And I then indicated certain very important events which, as I saw it, radically changed the outlook and almost "logically" determined that penicillin would be discovered in the 40s. In part, so as not to keep you in the dark, this change came about because of the discovery of the sulfa drugs. It had long been a policy of the German chemical works that all azo dyes and other chemical substances produced would be tested for their chemotherapeutic action, following the dreams of Ehrlich for the production of more chemical "magic bullets." Then, there was the surprising discovery by Wachsman and by Dubos, along with the work of Avery, indicating that not only were there new antibiotics ~~being discovered~~, but that there were naturally produced antibiotics. With the problems of war Florey was pushed by practical need in this new intellectual environment, almost one might say that there was an inexorable logic driving research in the direction of naturally produced antibiotics. Florey, you will recall, first began to explore the properties of lysozyme, and discovery of Fleming's which had never amounted to much, producing a substance with some antibiotic properties, derived from tears. That he would have encountered Fleming's observations on penicillium notatum follows in a natural or logical succession. There were, of course, certain other factors.

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What ^{has} interested me about this sequence is that it illustrated for me the way in which a discovery is actually brought to fruition in a matrix of scientific thinking and need (Florey in the late 30s and early 40s) whereas this did not occur at the earlier time. Recently, several books have appeared dealing with this whole episode, some of which explore this problem--but not deeply.

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I have been thinking of writing this up again, restating my own point of view, adding to it some of the recent information, but climaxing the whole analysis with a discussion by Florey himself. For the fact of the matter is that my old friend and teacher John F. Fulton was so interested in my book, and in this chapter, that he asked my permission to send it on to Florey. He did so, whereupon Florey studied my analysis carefully and wrote me a two-page letter containing his observations on my analysis.

I won't attempt to summarize Florey's letter to me, which I shall publish with a commentary, but I mention the example only to show you how your own concept of "postmature" discovery fits into and clarifies my own research and thinking.

Let me add, also, that I have ^{long} been collecting ~~much~~ material on prematurity. The fact is that many anticipations of scientific discoveries which are said to be "ahead of their time" would not be recognized at all but for the later discovery which gives a new meaning and intent to the original work. I have many examples of this phenomenon. It is clear to me that a careful reading of some of these premature discoveries would prove that the discoverer might not even have recognized what was implicit in ~~the~~ ^{his or her} work and revealed by the later discoveries. In other words, the prematurity becomes evident only after a recognized discovery changes our mode of thought, at which time we may perceive the seeds of this discovery in an earlier work. ©

There are, of course, some exceptions. I believe that Mendel is a very good example of such an exception, a real case of prematurity. It is quite the same for the anticipation of many of the features of today's computers by Charles Babbage in the middle of the nineteenth century. And, in the realm of technology, there is no question of the fact that Leonardo da Vinci anticipated many later principles and practices in the technological field.

The reasons why some very clear discoveries are not accepted may be a subject worthy of many books. Sometimes a discovery does not make an impact because it seems to go against the main current of interest and phenomena and so does not attract attention. This might very well be the case with regard to Mendel. You list some factors and I would agree with all of those--but I would add the significance of being an "insider" or an "outsider." We are all familiar with the rejection of a major breakthrough in organic chemistry because its author was in a veterinary school rather than a proper university. © For many years I was fascinated to learn how the work ~~was~~ ^{of J. Willard} Gibbs produced such an effect, since it was so radically different from what everyone else was doing in the field and since the main journal of publication was the Transactions of the Connecticut Academy of Science (a journal which very few people read regularly). One part of the mystery was solved when it

© see my P.S.

was found that Gibbs kept a list of people to whom he sent reprints--he made a wise choice, picking out those scientists who, by publications, had shown that they were interested in his area and competent enough to understand his work. At least four first-rate scientists took up Gibbs's ideas and disseminated them: James Clerk Maxwell in England, Wilhelm Ostwald in Germany, Rozeboom in Holland, and Pierre Duhem in France. These four men brought Gibbs's work to the attention and understanding of the scientific world, much as De Vries and Correns did for the work of Mendel at the turn of the last century. Among many aspects of prematurity, I believe that one must ~~separate out those~~ examples in which the scientist ~~was not really ahead of his time, or a pioneer~~ could not alter his "time," primarily because his work did not have the clarity and incisiveness to produce an effect, so that in actuality the discovery still remained to be made. You will see, from my letter, that I am really telling you about a series of articles and at least one book that I have in the offing, all of which are illuminated by your article.

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I am so glad to know that a more complete version will be published later on. I eagerly await a copy. And I cannot tell you how much I would look forward to the opportunity to discuss some of these questions further with each of you individually and with both of you further.

With best wishes,

Cordially yours,

Bernard

I. Bernard Cohen

IBC/cs

P.P.S. As I was writing this PS, I had just been rereading RKM (Soc.Th. & Soc.Str., 1968 edn, pp. 16-17) on this very topic.

P.S. In 1835 John Leslie wrote on some misplaced over-enthusiasm for precursors found only after a discovery: "Such facts are curious, and deserve attention; but every honourable mind must pity or scorn that invidious spirit with which some unhappy jackals hunt after imperfect and neglected anticipations, with a view of detracting from the merit of full discovery."